of momentum we have loss of visible kinetic energy, except when the coefficient of restitution = 1. This kinetic energy is transformed into the vibrational kinetic energy of sound and heat in general.

But cannot we have it partly transformed into potential energy by "soaring" against gravity? On this supposition we have the two laws, conservation of momentum where no forces act, and conservation of energy, holding. But we have visible kinetic energy lost and *partly transformed into potential energy with respect to the earth*, partly (as usual) into vibrational kinetic energy of sound and heat. [The *sound* is evident in the "singing" of the wings.]

It seems to me that the swooping referred to by your correspondent is only a matter of convenience to the bird, and does not really affect the mechanical question; and that the comparison to a kite (which is held by a string) is not very satisfactory. But from my own observation of sea-gulls I do not think one can say that all the manœuvres and turns of the bird in the air are performed without real muscular effort, though certainly without *flaps* of the wing; and if there be muscular effort there can be work done—against gravity in this case.

The above is only a suggestion. I wish to induce some more mathematical reader to write a clear answer on this interesting question. W. LARDEN

Cheltenham, November 8

The Photophone

ON reading the description published in NATURE, vol. xxiii. p. 15, of Prof. Graham Bell's wonderful discovery, the transmission of speech by light, I notice that in "the photophone" the varying of the intensity of the beam of light thrown on the receiving instrument is accomplished by the simple and ingenious means of allowing the sound-waves to beat on the back of a thin plane mirror. It seems to me, however, that this arrangement is not complete, and is open to some objection. As the plane mirror will, if provision be not made against it, become convex and concave alternately, it must, unless the vibrations be confined within very narrow limits, give in one vibration *two* periods of maximum and minimum illumination at the receiver, and therefore the received sounds, apparently, should be (assuming the periods between each maximum and minimum illumination to be of the same duration, which could never exactly occur) an octave higher than those transmitted. This I think follows from the fact that the rays from the mirror would be dispersed not only when convex, but also when concave, *after* they had passed the focus. If, therefore, the vibrations of the mirror are sufficiently great to bring its focus between the mirror and the receiving instrument, there would be a second point of minimum illumination. If however the mirror were made slightly convex, or were constrained by a spring or otherwise, this defect would be cured.

Curiously enough, *theoretically* "the photophone" is the more effective the greater the distance between the transmitter and the receiver, as the degree of variation of the intensity of light falling on the selenium will be, when perfectly adjusted, greater as the distance increases, and it is on this element that the intensity of the sound depends. A. R. MOLISON

Ffynone Club, Swansea, November 15

[Our correspondent is obviously right in supposing that with a beam of light focussed accurately upon the selenium receiver a *single* complete vibration of the transmitting disk would produce *two* periods of maximum and minimum illumination. This would not however be the case if the lenses were not set originally to exact focus, for then a displacement of the disk in one direction would scatter the rays more, while a displacement in the other would concentrate them more. In practice, we believe, exact focussing is never obtained or even attempted.—ED.]

Salts of Zinc

IN Roscoe and Schorlemmer, vol. ii. p. 264, it states: "The salts of zinc do *not* impart to the non-luminous flame any tint;" and on p. 258, "the metal burns with a bright *white* flame."

What then is the green colour imparted to the Bunsen flame by zinc sulphate due to? Also the green flame obtained by heating metallic zinc on charcoal before the blowpipe? S.

THE green tint referred to by "S." (*supra*) as imparted by zinc sulphate to the Bunsen flame is only observed whilst the water of crystallisation contained in the salt is being given off; the dry salt which remains imparts no colour to the flame. It therefore appears probable that the green colouration of the flame is caused by very finely divided particles of the salt being carried off into the upper part of the flame by the escaping water of crystallisation. These particles then become so intensely heated as to emit the peculiar greenish light and very likely suffer previous reduction by the carbon of the flame. Other zinc salts, especially the acetate, impart to the flame, when first heated, a greenish-blue tint resembling that observed when metallic zinc is burnt in the air, this being doubtlessly due to a partial reduction of the acetate. The characteristic zinc lines (6362 and 6099 in the red, and 4928, 4924, and 4911 in the blue) are not seen in the case of the salts or when the metal is burnt. A more correct description of the combustion of zinc than that referred to would be: "the metal burns with a bluish-white flame."

Chemical Laboratory, Owens College W. BOTT

THE WORKS OF CARL VON NÄGELI

THE beginning of the forties in the present century marks an important epoch in the history of botany. The "Naturphilosophie" which had for many years so banefully influenced the development of the science, was being routed by the energetic attacks of Schleiden. Botanists were becoming alive to the fact that if their study was to have a place as a science by the side of physics and of chemistry, it must be pursued by the inductive method; that speculation must give way to research, and, above all, that development must be studied before any conclusions could be drawn from the investigation of mature forms. The early discoveries of von Mohl, and the demonstration of the cellular structure of the tissues by Schleiden, were among the first fruits of this awakening. To this period belongs also Nägeli's first contribution to science—a paper on the Development of the Pollen (1842). The first sentence in the introduction shows how thoroughly Nägeli was imbued with the same spirit which possessed Schleiden. He says:—"The right knowledge of an object includes an acquaintance with its mature form and a study of its development: the one is dependent upon the other, and the one without the other is insufficient to afford a complete conception of the object." The actual observations detailed in the paper appear from the drawings to have been accurate, and they were an important addition to the knowledge of the subject; but their interpretation was so far influenced by Schleiden's theory of cell-formation, which was then prevalent, that the process of the development of the pollen grains is described as being one of free cell-formation.

In the year 1844 appeared the first number of the *Zeitschrift für wissenschaftliche Botanik*, edited—probably on account of the sympathy existing between them—by Schleiden and Nägeli. This short-lived periodical (1844 to 1846) was practically an organ for the publication of Nägeli's researches and for the expression of his views, for it does not contain a single contribution from Schleiden's pen. The first number opens with an article —a sort of confession of scientific faith—"On the Present Aims of Natural History, and especially of Botany," in which he gives an account of the actual state of botanical knowledge, and strongly urges the necessity of empirical study in order that the generalisations of the science might be in the future, not baseless speculations, but inductions resting upon a firm foundation of ascertained fact. The *Zeitschrift* further contains an important paper "On the Nuclei of Cells and the Formation and Growth of Cells," in which the process of free cell-formation, which Schleiden had asserted to be universal, is shown to be only one of the processes by which a multiplication of cells is effected; these processes are clearly defined and classified. This is followed by a number of researches on the morphology of the lower cryptogams, which are of interest inasmuch as they open up new lines of approach to the study of the complicated morphology of more highly

organised plants. Nägeli showed, for instance, that since in a unicellular alga (*Caulerpa*) a morphological differentiation of root, stem, and leaf is indicated, morphological is not dependent upon histological differentiation. He discovered also that in the organs of certain cryptogams growth is effected by the repeated segmentation of a single apical cell, and that this segmentation may take place always in one plane only (*Delesseria*), or in two or three planes (stem of *Echinomitrium*, *Phascum*, *Iungermannia*, leaves of mosses).

In the following year (1847) he published his work on "The Classification of the Algæ" ("Die Neueren Algensysteme"), which is of great value, partly on account of the acute criticisms on the various proposed classifications of this group of plants which it contains, but more particularly on account of the number of new facts concerning their structure and life-history which are contributed. The descriptions of *Valonia*, *Udotea*, and *Acetabularia* may be especially mentioned: it is shown that they have essentially the same structure as *Caulerpa*. The same praise may be awarded to another work, "The Genera of Unicellular Algæ," which appeared two years later.

The next publication of importance was the first number of the *Pflanzenphysiologische Untersuchungen*, and the most interesting of the papers which it contains is the one on the Primordial Utricle. Attention is directed to its presence in all living cells, to its influence upon the osmosis of substances in solution into or out of the cell, and to its activity in forming the cell-wall; in short, it is clearly shown to be the living portion of the cell. The second number did not appear until 1858, although the MS. was ready in 1855, the delay being due principally to Nägeli's removal from Freiburg to Zürich, and then again from Zürich to Munich on his acceptance of the Professorship of Botany in that University. Although it must have been vexatious, still the delay enabled Nägeli to extend his researches in various directions and thus contributed materially to make the great work on starch-granules one of the most complete monographs which was ever written on any subject. This second number is entirely taken up by this work, which gives an account of these bodies, including their structure, development, chemical composition, and physical properties, as well as their distribution in plants. It is a monument of patient, accurate investigation, devoted to a subject which appears, at first sight, to be of limited interest, but which ultimately suggested one of the most remarkable generalisations of modern times, namely, what is known as Nägeli's theory of the structure of organised bodies. The primary fact upon which this theory is based is the property which starch-granules have of swelling-up—that is, of absorbing a certain amount of fluid with a consequent increase of bulk—when treated with certain reagents (dilute acids and alkalies), and of diminishing in size in consequence of a loss of water when treated with other reagents (alcohol). From these phenomena he inferred that the starch-granule consists of solid particles, which are impenetrable by water, but which are capable of taking up a certain amount of water between them, and that the amount of this water may vary according to circumstances. When the granule is absolutely dry, these solid particles—to which he gave the name of *molecules*—apparently come into perfect contact, for the granule does not lose its transparency, which would be the case if air were included in its substance.

It may be remarked here parenthetically that the word *molecule* used by Nägeli to designate these solid particles has not the same sense as it has when it is used in chemistry; one of these molecules is probably an aggregation of chemical molecules. In order to avoid any possible confusion on this score Nägeli has substituted the word *micella* for molecule in his more recent works. The forces by which these micellæ, with their surrounding watery areas, are held together are, firstly, the attraction existing between the micellæ; and secondly, the attraction which exists between each micella and the water which surrounds it; the latter of these attractions must necessarily be greater than the former, but whereas the former varies inversely as the square of the distance, the latter must vary inversely as some higher power. Thus, if A represent the attraction between two micellæ, B the attraction between a micella and the water, and D the distance between two micellæ, the limit of swelling-up or imbibition will be reached when $\frac{B}{D^{2+x}} = \frac{A}{D^2}$. As to the form of the micellæ, it is evident that they are not spherical or oval, for in that case the starch-granules would necessarily contain air when dry, and further, the denser parts of them would have to contain at least 26 per cent. of water, whereas, as a matter of fact, they only contain 14 per cent. They must be therefore more or less polyhedral, but they are not equiaxial since the swelling-up does not take place equally in all directions.

By this theory it was found possible to explain satisfactorily certain difficult points of structure, such, for instance, as the stratification of starch-granules and the striation and stratification of cell-walls. All these depend upon the alternation, in one or more planes, of dense and less dense layers. The proportion of solid to fluid is greater in the dense than in the less dense layers, or, in the terms of Nägeli's theory, the relative size of the micellæ to the watery areas surrounding them is greater in the layers of greater density. Further, this theory affords a satisfactory explanation of the mode of growth of a cell-wall. It is easy to understand that when the limit of extensibility is nearly reached—that is, when the micellæ of the membrane are separated as far as possible—new micellæ can be deposited in the interstices, the extended condition of the membrane being thus rendered permanent. This mode of growth is commonly known as *growth by intussusception.*

This is the stage to which the development of the theory is brought in this work. In the year 1862 Nägeli published a paper in the *Proceedings* of the Bavarian Academy on the "Application of Polarised Light to the Study of the Structure of Plants," which advanced it very considerably. He found, in the first place, that organised structures, such as starch-granules or cell-walls, are doubly refractive, and that this property is not affected by causing them to increase or diminish in size in consequence either of the absorption or removal of water, or by mechanical stretching or pressure. From this he concluded that the double refraction is not a property of the organised structure as a whole, but that it belongs to each individual micella: hence these micellæ must be crystalline. Again, from the interference colours which these objects present when examined with polarised light, he ascertained that the crystalline micellæ have three axes of elasticity, that they must be bi-axial crystals; and further, by comparing the effect produced by the passage of polarised light through glass under various degrees of pressure, he arrived at the conclusion that the micellæ are so arranged in the membrane of which they form part that one of their axes of elasticity is perpendicular to the surface, whereas the other two axes lie in the plane of the membrane. In a subsequent paper contained in the same periodical, he shows that the crystals of proteid substance, which occur in various seeds and tubers, have the same molecular constitution as starch-granules and cell-walls. By close and acute reasoning from carefully observed facts, Nägeli has therefore succeeded in establishing this theory of the molecular constitution of organised bodies, a theory which satisfactorily explains many of the peculiarities of structure and properties which they present. There can be little doubt that it is justifiable to extend this theory to the explanation of the intimate structure of protoplasm; in fact, in his later publications Nägeli has asserted as much, and in

this he is supported by such authorities as Sachs and Strasburger; but it is impossible to say anything at present as to the form and arrangement of the micellæ of protoplasm beyond this, that they do not so act upon polarised light as to suggest that they are crystalline. Full details on this subject, as well as a vast amount of other information, is given in the treatise on the Microscope (second edition, 1877), which Nägeli wrote together with Schwendener; fortunately an English edition of this important work may soon be expected to appear.

In tracing the development of Nägeli's theory, it has been necessary to depart from the chronological order of his works. In the years between 1858 and 1868 he published his *Beiträge zur wissenschaftlichen Botanik*, which include several important works, for the most part anatomical. In the first number there is an elaborate paper on "The Arrangement of the Fibro-vascular Bundles and the Mode of Growth in the Stem and Root of Vascular Plants," which is important as containing a purely morphological classification of the different forms of tissue of which these organs consist. This is followed by a detailed account, in the fourth number, of the mode of growth in thickness and of the arrangement of the fibro-vascular bundles in the stem among the Sapindaceæ, and this number also contains Nägeli's well-known investigation into the mode of development and growth of roots, in which Leitgeb was associated with him. This publication has a further interest connected with it, in that Schwendener's first papers on what is now known as his Lichen-theory appeared in it.

During this period Nägeli frequently contributed papers (the *Botanische Mittheilungen*) on a variety of subjects of botanical interest to the *Proceedings* of the Bavarian Academy, an activity which continues up to the present time. Allusion has already been made to some of these, and it would be worth while, did space permit, to give an account of most of them. Among the more important the following may be mentioned:—"On the Sieve-Tubes of Cucurbita," "On the Proteid Crystalloids of the Brazil-nut," "On the Development of Varieties," "A Theory of Hybridisation." Of late years Nägeli has turned his attention more especially to the study of the chemical composition and vital processes of the lower Fungi, such as Yeast and Bacteria. Among the interesting results obtained is the discovery, in yeast-cells, of a ferment (invertin) which converts cane- into grape-sugar, and of peptones. But the real importance of these researches only became apparent on the publication of two larger works, viz.: "The Lower Fungi in their Relation to Infectious Disease" (1877), and "A Theory of Fermentation" (1879). It is of course impossible to give here anything like a satisfactory account of the contents of these two books. The first treats fully of the important part played by Bacteria in infection and contagion, showing, in fact, that these organisms are the causes and carriers of the various forms of disease. In the second, after an exhaustive account of the process of alcoholic fermentation has been given, a new theory of it is propounded, based, not upon chemical principles, like that of Liebig, but upon the principles of molecular physics. Fermentation is defined as being "the communication of the oscillations of the molecules, groups of atoms, and atoms of the substances composing the living protoplasm to the molecules of the fermentible substance, in consequence of which the equilibrium of the molecules of that substance is disturbed, and decomposition is the result." It is also pointed out that, in the case of yeast, the sugar is to some extent decomposed within the cells, but for the most part outside them.

Though this account of his works is but little more than an enumeration of them, yet it will suffice to show how important are Nägeli's contributions to botanical science in the departments of morphology, anatomy, and physiology, not merely as additions to the accumulated store of facts, but as new generalisations from those facts, and as opening up fields for future research.

SYDNEY H. VINES

PROF. TAIT ON THE FORMULA OF EVOLUTION[1]

ANOTHER point to which I ought thus early to direct your attention is the necessity for perfect definiteness of language in all truly scientific work. Want of definiteness may arise from habitual laziness, but it much more commonly indicates a desire to appear to know where knowledge is not. Avoid absolutely all so-called scientific writings in which (as Clerk-Maxwell said) the attempt is made to "give largeness of meaning" to a word by using it sometimes in one sense and sometimes in another. It is true that we may thus economise in our language, and avoid the necessity for introducing new and hard terms. But it would be a most expensive and pernicious economy. It is only a blockhead who could object to the use of a new term for a new idea.

Our only source of information in physical science is the evidence of our senses. To interpret truly this evidence, which is always imperfect and often wholly misleading, is one of the tasks set before Reason. It is only by the aid of reason that we can distinguish between what is physically objective, and what is merely subjective. Outside us there is no such thing as noise or brightness:—these no more exist in the aerial and ethereal motions, which are their objective cause, than does pain in the projectile which experience has taught us to avoid. You will find many prominent ideas, relics of a less enlightened age, from which Natural Philosophy has not yet wholly shaken itself free, which owed their existence solely to the confusion of the subjective with the objective.

With observation and experiment as our sole sources of information we have no right, in physical science, to introduce *à priori* reasoning. We may (unprofitably of course) speculate on what things might have been, but we must not dogmatise on what they ought to have been; we must simply try to discover what they are.

For aught that we can tell, the properties of matter, and physical laws in general, might have been other than we find them to be. How can any one of us tell whether his conscious self might not have been associated in life with the body of an Eskimo or of a New Zealander, instead of with what he (no doubt) considers its much preferable tenement? Speculations of such a kind must always be wholly unproductive and unprofitable, but for all that we cannot but allow that they are not intrinsically absurd.

Some years ago a critic of Mr. Herbert Spencer's Philosophy happened to quote from a book of mine the remark I have just made (that the properties of matter might have been other than we find them to be). Mr. Spencer's observation on this point is highly instructive. Had he not been a severely grave philosopher I should have taken it for a joke. He said, "Does this express an experimentally ascertained truth? If so, I invite Prof. Tait to describe the experiments."[2] Mr. Spencer has quite recently published a species of analytical inquiry[3] into my "mental peculiarities," "idiosyncrasies of thought," "habits of mind," "mental traits," and what not. From his illustrative quotations it appears that some or all of these are manifested wherever there are differences between myself and my critic in the points of view from which we regard the elements of science. Hence they are not properly personal questions at all, but

[1] Part of an Introductory Lecture delivered October 26, 1880.

[2] In my letter (NATURE, vol. ix. p. 402) will be found an illustrative anecdote, which Mr. Spencer declares to be "*not to the point*." A great scientific man, to whom I showed the correspondence, remarked that Mr. Spencer must be the only man in England who could not see the perfect appositeness of the anecdote.

[3] *Appendix to First Principles, dealing with Criticisms.* (Williams and Norgate, 1880.)